Against Method

Fourth Edition

Paul Feyerabend

VERSO
London • New York

Analytical Index

Being a Sketch of the Main Argument

Introduction

Science is an essentially anarchic enterprise: theoretical anarchism is more humanitarian and more likely to encourage progress than its law-and-order alternatives.

> Ordnung ist heutzutage meistens dort, wo nichts ist. Es ist eine Mangelerscheinung.
>
> *Brecht*

The following essay is written in the conviction that *anarchism*, while perhaps not the most attractive *political* philosophy, is certainly excellent medicine for *epistemology*, and for the *philosophy of science*.

The reason is not difficult to find.

'History generally, and the history of revolution in particular, is always richer in content, more varied, more many-sided, more lively and subtle than even' the best historian and the best methodologist can imagine.[1] History is full of 'accidents and conjunctures and curious juxtapositions of events'[2] and it demonstrates to us the 'complexity of human change and the unpredictable character of the ultimate consequences of any given act or decision of men'.[3] Are we really to believe that the naive and simple-minded rules which methodologists take as their guide are capable of accounting for such a 'maze of interactions'?[4] And is it not

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1. 'History as a whole, and the history of revolutions in particular, is always richer in content, more varied, more multiform, more lively and ingenious than is imagined by even the best parties, the most conscious vanguards of the most advanced classes' (V.I. Lenin, 'Left-Wing Communism – An Infantile Disorder', *Selected Works*, Vol. 3, London, 1967, p. 401). Lenin is addressing parties and revolutionary vanguards rather than scientists and methodologists; the lesson, however, is the same. Cf. footnote 5.

2. Herbert Butterfield, *The Whig Interpretation of History*, New York, 1965, p. 66.

3. Ibid., p. 21.

4. Ibid., p. 25, cf. Hegel, *Philosophie der Geschichte*, *Werke*, Vol. 9, ed. Edward Gans, Berlin, 1837, p. 9: 'But what experience and history teach us is this, that nations and governments have never learned anything from history, or acted according to rules that might have derived from it. Every period has such peculiar circumstances, is in such an individual state, that decisions will have to be made, and decisions *can* only be made, in it and out of it.' – 'Very clever'; 'shrewd and very clever'; 'NB' writes Lenin in his marginal notes to this passage. (*Collected Works*, Vol. 38, London, 1961, p. 307.)

clear that successful *participation* in a process of this kind is possible only for a ruthless opportunist who is not tied to any particular philosophy and who adopts whatever procedure seems to fit the occasion?

This is indeed the conclusion that has been drawn by intelligent and thoughtful observers. 'Two very important practical conclusions follow from this [character of the historical process],' writes Lenin,[5] continuing the passage from which I have just quoted. 'First, that in order to fulfil its task, the revolutionary class [i.e. the class of those who want to change either a part of society such as science, or society as a whole] must be able to master *all* forms or aspects of social activity without exception [it must be able to understand, and to apply, not only one particular methodology, but any methodology, and any variation thereof it can imagine] . . .; second [it] must be ready to pass from one to another in the quickest and most unexpected manner.' 'The external conditions', writes Einstein,[6] 'which are set for [the scientist] by the facts of experience do not permit him to let himself be too much restricted, in the construction of his conceptual world, by the adherence to an epistemological system. He, therefore, must appear to the systematic epistemologist as a type of unscrupulous opportunist. . . .' A complex medium containing surprising and unforeseen developments demands complex procedures and defies analysis on the basis of rules which

5. Ibid. We see here very clearly how a few substitutions can turn a political lesson into a lesson for *methodology*. This is not at all surprising. Methodology and politics are both means for moving from one historical stage to another. We also see how an individual, such as Lenin, who is not intimidated by traditional boundaries and whose thought is not tied to the ideology of a particular profession, can give useful advice to everyone, philosophers of science included. In the 19th century the idea of an elastic and historically informed methodology was a matter of course. Thus Ernst Mach wrote in his book *Erkenntnis und Irrtum*, Neudruck, Wissenschaftliche Buchgesell-schaft, Darmstadt, 1980, p. 200: 'It is often said that research cannot be taught. That is quite correct, in a certain sense. The schemata of *formal* logic and of *inductive* logic are of little use for the intellectual situations are never exactly the same. But the examples of great scientists are very suggestive.' They are not suggestive because we can abstract rules from them and subject future research to their jurisdiction; they are suggestive because they make the mind nimble and capable of inventing entirely new research traditions. For a more detailed account of Mach's philosophy see my essay *Farewell to Reason*, London, 1987, Chapter 7, as well as Vol. 2, Chapters 5 and 6 of my *Philosophical Papers*, Cambridge, 1981.

6. Albert Einstein, *Albert Einstein: Philosopher Scientist*, ed. P.A. Schilpp, New York, 1951, pp. 683f.

have been set up in advance and without regard to the ever-changing conditions of history.

Now it is, of course, possible to simplify the medium in which a scientist works by simplifying its main actors. The history of science, after all, does not just consist of facts and conclusions drawn from facts. It also contains ideas, interpretations of facts, problems created by conflicting interpretations, mistakes, and so on. On closer analysis we even find that science knows no 'bare facts' at all but that the 'facts' that enter our knowledge are already viewed in a certain way and are, therefore, essentially ideational. This being the case, the history of science will be as complex, chaotic, full of mistakes, and entertaining as the ideas it contains, and these ideas in turn will be as complex, chaotic, full of mistakes, and entertaining as are the minds of those who invented them. Conversely, a little brainwashing will go a long way in making the history of science duller, simpler, more uniform, more 'objective' and more easily accessible to treatment by strict and unchangeable rules.

Scientific education as we know it today has precisely this aim. It simplifies 'science' by simplifying its participants: first, a domain of research is defined. The domain is separated from the rest of history (physics, for example, is separated from metaphysics and from theology) and given a 'logic' of its own. A thorough training in such a 'logic' then conditions those working in the domain; it makes *their actions* more uniform and it freezes large parts of the *historical process* as well. Stable 'facts' arise and persevere despite the vicissitudes of history. An essential part of the training that makes such facts appear consists in the attempt to inhibit intuitions that might lead to a blurring of boundaries. A person's religion, for example, or his metaphysics, or his sense of humour (his *natural* sense of humour and not the inbred and always rather nasty kind of jocularity one finds in specialized professions) must not have the slightest connection with his scientific activity. His imagination is restrained, and even his language ceases to be his own. This is again reflected in the nature of scientific 'facts' which are experienced as being independent of opinion, belief, and cultural background.

It is thus *possible* to create a tradition that is held together by strict rules, and that is also successful to some extent. But is it *desirable* to support such a tradition to the exclusion of everything else? Should we transfer to it the sole rights for dealing in knowledge, so that any result that has been obtained by other methods is at once ruled out of court?

And did scientists ever remain within the boundaries of the traditions they defined in this narrow way? These are the questions I intend to ask in the present essay. And to these questions my answer will be a firm and resounding NO.

There are two reasons why such an answer seems to be appropriate. The first reason is that the world which we want to explore is a largely unknown entity. We must, therefore, keep our options open and we must not restrict ourselves in advance. Epistemological prescriptions may look splendid when compared with other epistemological prescriptions, or with general principles – but who can guarantee that they are the best way to discover, not just a few isolated 'facts', but also some deep-lying secrets of nature? The second reason is that a scientific education as described above (and as practised in our schools) cannot be reconciled with a humanitarian attitude. It is in conflict 'with the cultivation of individuality which alone produces, or can produce, well-developed human beings';[7] it 'maims by compression, like a Chinese lady's foot, every part of human nature which stands out prominently, and tends to make a person markedly different in outline'[8] from the ideals of rationality that happen to be fashionable in science, or in the philosophy of science. The attempt to increase liberty, to lead a full and rewarding life, and the corresponding attempt to discover the secrets of nature and of man, entails, therefore, the rejection of all universal standards and of all rigid traditions. (Naturally, it also entails the rejection of a large part of contemporary science.)

It is surprising to see how rarely the stultifying effect of 'the Laws of Reason' or of scientific practice is examined by professional anarchists. Professional anarchists oppose any kind of restriction and they demand that the individual be permitted to develop freely, unhampered by laws, duties or obligations. And yet they swallow without protest all the severe standards which scientists and logicians impose upon research and upon any kind of knowledge-creating and knowledge-changing activity. Occasionally, the laws of scientific method, or what are thought to be the laws of scientific method by a particular writer, are even integrated into anarchism itself. 'Anarchism is a world concept based upon a mechanical

7. John Stuart Mill, 'On Liberty', in *The Philosophy of John Stuart Mill*, ed. Marshall Cohen, New York, 1961, p. 258.

8. Ibid., p. 265.

explanation of all phenomena,' writes Kropotkin.[9] 'Its method of investigation is that of the exact natural sciences ... the method of induction and deduction.' 'It is not so clear,' writes a modern 'radical' professor at Columbia,[10] 'that scientific research demands an absolute freedom of speech and debate. Rather the evidence suggests that certain kinds of unfreedom place no obstacle in the way of science. ...'

There are certainly some people to whom this is 'not so clear'. Let us, therefore, start with our outline of an anarchistic methodology and a corresponding anarchistic science. There is no need to fear that the diminished concern for law and order in science and society that characterizes an anarchism of this kind will lead to chaos. The human nervous system is too well organized for that.[11] There may, of course, come a time when it will be necessary to give reason a temporary advantage and when it will be wise to defend its rules to the exclusion of everything else. I do not think that we are living in such a time today.[12]

9. Peter Alexeivich Kropotkin, 'Modern Science and Anarchism', *Kropotkin's Revolutionary Pamphlets*, ed. R.W. Baldwin, New York, 1970, pp. 150–2. 'It is one of Ibsen's great distinctions that nothing was valid for him but science.' B. Shaw, *Back to Methuselah*, New York, 1921, p. xcvii. Commenting on these and similar phenomena Strindberg writes (*Antibarbarus*): 'A generation that had the courage to get rid of God, to crush the state and church, and to overthrow society and morality, still bowed before Science. And in Science, where freedom ought to reign, the order of the day was "believe in the authorities or off with your head".'

10. R.P. Wolff, *The Poverty of Liberalism*, Boston, 1968, p. 15. For a criticism of Wolff see footnote 52 of my essay 'Against Method', in *Minnesota Studies in the Philosophy of Science*, Vol. 4, Minneapolis, 1970.

11. Even in undetermined and ambiguous situations, uniformity of action is soon achieved and adhered to tenaciously. See Muzafer Sherif, *The Psychology of Social Norms*, New York, 1964.

12. This was my opinion in 1970 when I wrote the first version of this essay. Times have changed. Considering some tendencies in US education ('politically correct' academic menus, etc.), in philosophy (postmodernism) and in the world at large I think that reason should now be given greater weight not because it is and always was fundamental but because it seems to be needed, in circumstances that occur rather frequently today (but may disappear tomorrow), to create a more humane approach.

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This is shown both by an examination of historical episodes and by an abstract analysis of the relation between idea and action. The only principle that does not inhibit progress is: anything goes.

The idea of a method that contains firm, unchanging, and absolutely binding principles for conducting the business of science meets considerable difficulty when confronted with the results of historical research. We find, then, that there is not a single rule, however plausible, and however firmly grounded in epistemology, that is not violated at some time or other. It becomes evident that such violations are not accidental events, they are not results of insufficient knowledge or of inattention which might have been avoided. On the contrary, we see that they are necessary for progress. Indeed, one of the most striking features of recent discussions in the history and philosophy of science is the realization that events and developments, such as the invention of atomism in antiquity, the Copernican Revolution, the rise of modern atomism (kinetic theory; dispersion theory; stereochemistry; quantum theory), the gradual emergence of the wave theory of light, occurred only because some thinkers either *decided* not to be bound by certain 'obvious' methodological rules, or because they *unwittingly broke* them.

This liberal practice, I repeat, is not just a *fact* of the history of science. It is both reasonable and *absolutely necessary* for the growth of knowledge. More specifically, one can show the following: given any rule, however 'fundamental' or 'rational', there are always circumstances when it is advisable not only to ignore the rule, but to adopt its opposite. For example, there are circumstances when it is advisable to introduce, elaborate, and defend *ad hoc* hypotheses, or hypotheses which contradict well-established and generally accepted experimental results, or hypotheses whose content is smaller than the content of the existing and empirically adequate alternative, or self-inconsistent hypotheses, and so on.[1]

1. One of the few thinkers to understand this feature of the development of knowledge was Niels Bohr: '. . . he would never try to outline any finished picture, but would patiently go through all the phases of the development of a problem, starting from some apparent paradox, and gradually leading to its elucidation. In fact, he never

There are even circumstances – and they occur rather frequently – when *argument* loses its forward-looking aspect and becomes a hindrance to progress. Nobody would claim that the teaching of *small children* is exclusively a matter of argument (though argument may enter into it, and should enter into it to a larger extent than is customary), and almost everyone now agrees that what looks like a result of reason – the mastery of a language, the existence of a richly articulated perceptual world, logical ability – is due partly to indoctrination and partly to a process of *growth* that proceeds with the force of natural law. And where arguments do seem to have an effect, this is more often due to their *physical repetition* than to their *semantic content.*

Having admitted this much, we must also concede the possibility of non-argumentative growth in the *adult* as well as in (the theoretical parts of) *institutions* such as science, religion, prostitution, and so on. We certainly cannot take it for granted that what is possible for a small child – to acquire new modes of behaviour on the slightest provocation, to

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regarded achieved results in any other light than as starting points for further exploration. In speculating about the prospects of some line of investigation, he would dismiss the usual consideration of simplicity, elegance or even consistency with the remark that such qualities can only be properly judged *after* [my italics] the event. . . .' L. Rosenfeld in *Niels Bohr. His Life and Work as seen by his Friends and Colleagues*, S. Rosental (ed.), New York, 1967, p. 117. Now science is never a completed process, therefore it is always 'before' the event. Hence simplicity, elegance or consistency are *never* necessary conditions of (scientific) practice.

Considerations such as these are usually criticized by the childish remark that a contradiction 'entails' everything. But contradictions do not 'entail' anything unless people use them in certain ways. And people will use them as entailing everything only if they accept some rather simple-minded rules of derivation. Scientists proposing theories with logical faults and obtaining interesting results with their help (for example: the results of early forms of the calculus; of a geometry where lines consist of points, planes of lines and volumes of planes; the predictions of the older quantum theory and of early forms of the quantum theory of radiation – and so on) evidently proceed according to different rules. The criticism therefore falls back on its authors unless it can be shown that a logically decontaminated science has better results. Such a demonstration is impossible. Logically perfect versions (if such versions exist) usually arrive only long after the imperfect versions have enriched science by their contributions. For example, wave mechanics was not a 'logical reconstruction' of preceding theories; it was an attempt to preserve their achievements and to solve the physical problems that had arisen from their use. Both the achievements and the problems were produced in a way very different from the ways of those who want to subject everything to the tyranny of 'logic'.

slide into them without any noticeable effort – is beyond the reach of his elders. One should rather expect that catastrophic changes in the physical environment, wars, the breakdown of encompassing systems of morality, political revolutions, will transform adult reaction patterns as well, including important patterns of argumentation. Such a transformation may again be an entirely natural process and the only function of a rational argument may lie in the fact that it increases the mental tension that preceded *and caused* the behavioural outburst.

Now, if there are events, not necessarily arguments, which *cause* us to adopt new standards, including new and more complex forms of argumentation, is it then not up to the defenders of the *status quo* to provide, not just counter-arguments, but also contrary *causes*? ('Virtue without terror is ineffective,' says Robespierre.) And if the old forms of argumentation turn out to be too weak a cause, must not these defenders either give up or resort to stronger and more 'irrational' means? (It is very difficult, and perhaps entirely impossible, to combat the effects of brainwashing by argument.) Even the most puritanical rationalist will then be forced to stop reasoning and to use *propaganda* and *coercion*, not because some of his *reasons* have ceased to be valid, but because the *psychological conditions* which make them effective, and capable of influencing others, have disappeared. And what is the use of an argument that leaves people unmoved?

Of course, the problem never arises quite in this form. The teaching of standards and their defence never consists merely in putting them before the mind of the student and making them as *clear* as possible. The standards are supposed to have maximal *causal efficacy* as well. This makes it very difficult indeed to distinguish between the *logical force* and the *material effect* of an argument. Just as a well-trained pet will obey his master no matter how great the confusion in which he finds himself, and no matter how urgent the need to adopt new patterns of behaviour, so in the very same way a well-trained rationalist will obey the mental image of *his* master, he will conform to the standards of argumentation he has learned, he will adhere to these standards no matter how great the confusion in which he finds himself, and he will be quite incapable of realizing that what he regards as the 'voice of reason' is but a *causal after-effect* of the training he had received. He will be quite unable to discover that the appeal to reason to which he succumbs so readily is nothing but a *political manoeuvre*.

That interests, forces, propaganda and brainwashing techniques play a much greater role than is commonly believed in the growth of our knowledge and in the growth of science, can also be seen from an analysis of the *relation between idea and action.* It is often taken for granted that a clear and distinct understanding of new ideas precedes, and should precede, their formulation and their institutional expression. *First,* we have an idea, or a problem, *then* we act, i.e. either speak, or build, or destroy. Yet this is certainly not the way in which small children develop. They use words, they combine them, they play with them, until they grasp a meaning that has so far been beyond their reach. And the initial playful activity is an essential prerequisite of the final act of understanding. There is no reason why this mechanism should cease to function in the adult. We must expect, for example, that the *idea* of liberty could be made clear only by means of the very same actions, which were supposed to *create* liberty. Creation of a *thing,* and creation plus full understanding of a *correct idea* of the thing, *are very often parts of one and the same indivisible process* and cannot be separated without bringing the process to a stop. The process itself is not guided by a well-defined programme, and cannot be guided by such a programme, for it contains the conditions for the realization of all possible programmes. It is guided rather by a vague urge, by a 'passion' (Kierkegaard). The passion gives rise to specific behaviour which in turn creates the circumstances and the ideas necessary for analysing and explaining the process, for making it 'rational'.

The development of the Copernican point of view from Galileo to the 20th century is a perfect example of the situation I want to describe. We start with a strong belief that runs counter to contemporary reason and contemporary experience. The belief spreads and finds support in other beliefs which are equally unreasonable, if not more so (law of inertia; the telescope). Research now gets deflected in new directions, new kinds of instruments are built, 'evidence' is related to theories in new ways until there arises an ideology that is rich enough to provide independent arguments for any particular part of it and mobile enough to find such arguments whenever they seem to be required. We can say today that Galileo was on the right track, for his persistent pursuit of what once seemed to be a silly cosmology has by now created the material needed to defend it against all those who will accept a view only if it is told in a certain way and who will trust it only if it contains certain magical phrases, called

'observational reports'. And this is not an exception – it is the normal case: theories become clear and 'reasonable' only *after* incoherent parts of them have been used for a long time. Such unreasonable, nonsensical, unmethodical foreplay thus turns out to be an unavoidable precondition of clarity and of empirical success.

Now, when we attempt to describe and to understand developments of this kind in a general way, we are, of course, obliged to appeal to the existing forms of speech which do not take them into account and which must be distorted, misused, beaten into new patterns in order to fit unforeseen situations (without a constant misuse of language there cannot be any discovery, any progress). 'Moreover, since the traditional categories are the gospel of everyday thinking (including ordinary scientific thinking) and of everyday practice, [such an attempt at understanding] in effect presents rules and forms of false thinking and action – false, that is, from the standpoint of (scientific) common sense.'[2] This is how *dialectical thinking* arises as a form of thought that 'dissolves into nothing the detailed determinations of the understanding',[3] formal logic included.

(Incidentally, it should be pointed out that my frequent use of such words as 'progress', 'advance', 'improvement', etc., does not mean that I claim to possess special knowledge about what is good and what is bad in the sciences and that I want to impose this knowledge upon my readers. *Everyone can read the terms in his own way* and in accordance with the tradition to which he belongs. Thus for an empiricist, 'progress' will mean transition to a theory that provides direct empirical tests for most of its basic assumptions. Some people believe the quantum theory to be a theory of this kind. For others, 'progress' may mean unification and harmony, perhaps even at the expense of empirical adequacy. This is how Einstein viewed the general theory of relativity. *And my thesis is that anarchism helps to achieve progress in any one of the senses one cares to choose.* Even a law-and-order science will succeed only if anarchistic moves are occasionally allowed to take place.)

It is clear, then, that the idea of a fixed method, or of a fixed theory of rationality, rests on too naive a view of man and his social surroundings. To those who look at the rich material provided by history, and who are

2. Herbert Marcuse, *Reason and Revolution*, London, 1941, p. 130.
3. Hegel, *Wissenschaft der Logik*, Vol. 1, Hamburg, 1965, p. 6.

not intent on impoverishing it in order to please their lower instincts, their craving for intellectual security in the form of clarity, precision, 'objectivity', 'truth', it will become clear that there is only one principle that can be defended under *all* circumstances and in all stages of human development. It is the principle: *anything goes*.

This abstract principle must now be examined and explained in concrete detail.

For example, we may use hypotheses that contradict well-confirmed theories and/or well-established experimental results. We may advance science by proceeding counterinductively.

Examining the principle in concrete detail means tracing the consequences of 'counterrules' which oppose familiar rules of the scientific enterprise. To see how this works, let us consider the rule that it is 'experience', or the 'facts', or 'experimental results' which measure the success of our theories, that agreement between a theory and the 'data' favours the theory (or leaves the situation unchanged) while disagreement endangers it, and perhaps even forces us to eliminate it. This rule is an important part of all theories of confirmation and corroboration. It is the essence of empiricism. The 'counterrule' corresponding to it advises us to introduce and elaborate hypotheses which are inconsistent with well-established theories and/or well-established facts. It advises us to proceed *counterinductively.*

The counterinductive procedure gives rise to the following questions: Is counterinduction more reasonable than induction? Are there circumstances favouring its use? What are the arguments for it? What are the arguments against it? Is perhaps induction always preferable to counterinduction? And so on.

These questions will be answered in two steps. I shall first examine the counterrule that urges us to develop hypotheses inconsistent with accepted and highly confirmed *theories.* Later on I shall examine the counterrule that urges us to develop hypotheses inconsistent with well-established *facts.* The results may be summarized as follows.

In the first case it emerges that the evidence that might refute a theory can often be unearthed only with the help of an incompatible alternative: the advice (which goes back to Newton and which is still very popular today) to use alternatives only when refutations have already discredited the orthodox theory puts the cart before the horse. Also, some of the most important formal properties of a theory are found by contrast, and not by analysis. A scientist who wishes to maximize the empirical content of the views he holds and who wants to understand them as clearly as he possibly can must therefore introduce other views; that is, he must adopt a *pluralistic methodology.* He must compare ideas with other ideas rather than with 'experience' and he must try

to improve rather than discard the views that have failed in the competition. Proceeding in this way he will retain the theories of man and cosmos that are found in Genesis, or in the Pimander, he will elaborate them and use them to measure the success of evolution and other 'modern' views. He may then discover that the theory of evolution is not as good as is generally assumed and that it must be supplemented, or entirely replaced, by an improved version of Genesis. Knowledge so conceived is not a series of self-consistent theories that converges towards an ideal view; it is not a gradual approach to the truth. It is rather an ever increasing *ocean of mutually incompatible alternatives*, each single theory, each fairy-tale, each myth that is part of the collection forcing the others into greater articulation and all of them contributing, via this process of competition, to the development of our consciousness. Nothing is ever settled, no view can ever be omitted from a comprehensive account. Plutarch or Diogenes Laertius, and not Dirac or von Neumann, are the models for presenting a knowledge of this kind in which the *history* of a science becomes an inseparable part of the science itself – it is essential for its further *development* as well as for giving *content* to the theories it contains at any particular moment. Experts and laymen, professionals and dilettanti, truth-freaks and liars – they all are invited to participate in the contest and to make their contribution to the enrichment of our culture. The task of the scientist, however, is no longer 'to search for the truth', or 'to praise god', or 'to systematize observations', or 'to improve predictions'. These are but side effects of an activity to which his attention is now mainly directed and which is *'to make the weaker case the stronger'* as the sophists said, *and thereby to sustain the motion of the whole.*

The second 'counterrule' which favours hypotheses inconsistent with *observations, facts and experimental results*, needs no special defence, for there is not a single interesting theory that agrees with all the known facts in its domain. The question is, therefore, not whether counterinductive theories should be *admitted* into science; the question is, rather, whether the *existing* discrepancies between theory and fact should be increased, or diminished, or what else should be done with them.

To answer this question it suffices to remember that observational reports, experimental results, 'factual' statements, either *contain* theoretical assumptions or *assert* them by the manner in which they are used. (For this point cf. the discussion of natural interpretations in Chapters 6ff.) Thus our habit of saying 'the table is brown' when we view it under normal circumstances, with our senses in good order, but 'the table seems to be brown' when either the lighting conditions are poor or when we feel unsure

in our capacity of observation expresses the belief that there are familiar circumstances when our senses are capable of seeing the world 'as it really is' and other, equally familiar circumstances, when they are deceived. It expresses the belief that some of our sensory impressions are veridical while others are not. We also take it for granted that the material medium between the object and us exerts no distorting influence, and that the physical entity that establishes the contact – light – carries a true picture. All these are abstract, and highly doubtful, assumptions which shape our view of the world without being accessible to a direct criticism. Usually, we are not even aware of them and we recognize their effects only when we encounter an entirely different cosmology: prejudices are found by contrast, not by analysis. The material which the *scientist* has at his disposal, his most sublime theories and his most sophisticated techniques included, is structured in exactly the same way. It again contains principles which are not known and which, if known, would be extremely hard to test. (As a result, a theory may clash with the evidence not because it is not correct, but because the evidence is contaminated.)

Now – how can we possibly examine something we are using all the time? How can we analyse the terms in which we habitually express our most simple and straightforward observations, and reveal their presuppositions? How can we discover the kind of world we presuppose when proceeding as we do?

The answer is clear: we cannot discover it from the *inside*. We need an *external* standard of criticism, we need a set of alternative assumptions or, as these assumptions will be quite general, constituting, as it were, an entire alternative world, *we need a dream-world in order to discover the features of the real world we think we inhabit* (and which may actually be just another dream-world). The first step in our criticism of familiar concepts and procedures, the first step in our criticism of 'facts', must therefore be an attempt to break the circle. We must invent a new conceptual system that suspends, or clashes with, the most carefully established observational results, confounds the most plausible theoretical principles, and introduces perceptions that cannot form part of the existing perceptual world.[1] This step is again counterinductive.

1. 'Clashes' or 'suspends' is meant to be more general than 'contradicts'. I shall say that a set of ideas or actions 'clashes' with a conceptual system if it is either inconsistent with it, or makes the system appear absurd. For details see Chapter 16 below.

Counterinduction is, therefore, always reasonable and it has always a chance of success.

In the following seven chapters, this conclusion will be developed in greater detail and it will be elucidated with the help of historical examples. One might therefore get the impression that I recommend a new methodology which replaces induction by counterinduction and uses a multiplicity of theories, metaphysical views, fairy-tales instead of the customary pair theory/observation.[2] This impression would certainly be mistaken. My intention is not to replace one set of general rules by another such set: my intention is, rather, to convince the reader that *all methodologies, even the most obvious ones, have their limits.* The best way to show this is to demonstrate the limits and even the irrationality of some rules which she, or he, is likely to regard as basic. In the case of induction (including induction by falsification) this means demonstrating how well the counterinductive procedure can be supported by argument. Always remember that the demonstrations and the rhetorics used do not express any 'deep convictions' of mine. They merely show how easy it is to lead people by the nose in a rational way. An anarchist is like an undercover agent who plays the game of Reason in order to undercut the authority of Reason (Truth, Honesty, Justice, and so on).[3]

2. This is how Professor Ernan McMullin interpreted some earlier papers of mine. See 'A Taxonomy of the Relations between History and Philosophy of Science', *Minnesota Studies*, Vol. 5, Minneapolis, 1971.

3. 'Dada', says Hans Richter in *Dada: Art and Anti-Art*, 'not only had no programme, it was against all programmes.' This does not exclude the skilful defence of programmes to show the chimerical character of any defence, however 'rational'. (In the same way an actor or a playwright could produce all the outer manifestations of 'deep love' in order to debunk the idea of 'deep love' itself. Example: Pirandello.)

3

The consistency condition which demands that new hypotheses agree with accepted theories *is unreasonable because it preserves the older theory, and not the better theory. Hypotheses contradicting well-confirmed theories give us evidence that cannot be obtained in any other way. Proliferation of theories is beneficial for science, while uniformity impairs its critical power. Uniformity also endangers the free development of the individual.*

In this chapter I shall present more detailed arguments for the 'counterrule' that urges us to introduce hypotheses which are *inconsistent* with well-established *theories*. The arguments will be indirect. They will start with a criticism of the demand that new hypotheses must be consistent with such theories. This demand will be called the *consistency condition*.[1]

Prima facie, the case of the consistency condition can be dealt with in a few words. It is well known (and has also been shown in detail by Duhem) that Newton's mechanics is inconsistent with Galileo's law of free fall and with Kepler's laws; that statistical thermodynamics is inconsistent with the second law of the phenomenological theory; that wave optics is inconsistent with geometrical optics; and so on.[2] Note that what is being asserted here is *logical* inconsistency; it may well be that the differences of prediction are too small to be detected by experiment. Note also that what is being asserted is not the *inconsistency* of, say, Newton's *theory* and Galileo's law, but rather the inconsistency of *some consequences* of Newton's theory in the domain of validity of Galileo's law, and Galileo's law. In the last case, the situation is especially clear. Galileo's law asserts that the acceleration of free fall is a constant, whereas application of Newton's theory to the surface of the earth gives an acceleration that is not constant but *decreases* (although imperceptibly) with the distance from the centre of the earth.

To speak more abstractly: consider a theory T′ that successfully describes the situation inside domain D′. T′ agrees with a *finite* number

1. The consistency condition goes back to Aristotle at least. It plays an important part in Newton's philosophy (though Newton himself constantly violated it). It is taken for granted by many 20th-century scientists and philosophers of science.

2. Pierre Duhem, *The Aim and Structure of Physical Theory*, New York, 1962, pp. 180ff.

of observations (let their class be F) and it agrees with these observations inside a margin M of error. Any alternative that contradicts T′ outside F and inside M is supported by exactly the same observations and is therefore acceptable if T′ was acceptable (I shall assume that F are the only observations made). The consistency condition is much less tolerant. It eliminates a theory or a hypothesis not because it disagrees with the facts; it eliminates it because it disagrees with another theory, with a theory, moreover, whose confirming instances it shares. It thereby makes the as yet untested part of that theory a measure of validity. The only difference between such a measure and a more recent theory is age and familiarity. Had the younger theory been there first, then the consistency condition would have worked in its favour. 'The *first* adequate theory has the right of priority over equally adequate aftercomers.'[3] In this respect the effect of the consistency condition is rather similar to the effect of the more traditional methods of transcendental deduction, analysis of essences, phenomenological analysis, linguistic analysis. It contributes to the preservation of the old and familiar not because of any inherent advantage in it but because it is old and familiar. This is not the only instance where on closer inspection a rather surprising similarity emerges between modern empiricism and some of the school philosophies it attacks.

Now it seems to me that these brief considerations, although leading to an interesting *tactical* criticism of the consistency condition, and to some first shreds of support for counterinduction, do not yet go to the heart of the matter. They show that an alternative to the accepted point of view which shares its confirming instances cannot be *eliminated* by factual reasoning. They do not show that such an alternative is *acceptable*; and even less do they show that it *should be used*. It is bad enough, a defender of the consistency condition might point out, that the accepted view does not possess full empirical support. Adding new theories of *an equally unsatisfactory character* will not improve the situation; nor is there much sense in trying to *replace* the accepted theories by some of their possible alternatives. Such replacement will be no easy matter. A new formalism may have to be learned and familiar problems may have to be calculated in a new way. Textbooks must be rewritten, university curricula readjusted,

3. C. Truesdell, 'A Program Toward Rediscovering the Rational Mechanics of the Age of Reason', *Archives for the History of Exact Sciences*, Vol. 1, p. 14.

experimental results reinterpreted. And what will be the result of all the effort? Another theory which from an empirical standpoint has no advantage whatsoever over and above the theory it replaces. The only real improvement, so the defender of the consistency condition will continue, derives from the *addition of new facts*. Such new facts will either support the current theories, or they will force us to modify them by indicating precisely where they go wrong. In both cases they will precipitate real progress and not merely arbitrary change. The proper procedure must therefore consist in the confrontation of the accepted point of view with as many relevant facts as possible. The exclusion of alternatives is then simply a measure of expediency: their invention not only does not help, it even hinders progress by absorbing time and manpower that could be devoted to better things. The consistency condition eliminates such fruitless discussion and it forces the scientist to concentrate on the facts which, after all, are the only acceptable judges of a theory. This is how the practising scientist will defend his concentration on a single theory to the exclusion of empirically possible alternatives.

It is worthwhile repeating the reasonable core of this argument. Theories should not be changed unless there are pressing reasons for doing so. The only pressing reason for changing a theory is disagreement with facts. Discussion of incompatible facts will therefore lead to progress. Discussion of incompatible hypotheses will not. Hence, it is sound procedure to increase the number of relevant facts. It is not sound procedure to increase the number of factually adequate, but incompatible, alternatives. One might wish to add that formal improvements such as increased elegance, simplicity, generality, and coherence should not be excluded. But once these improvements have been carried out, the collection of facts for the purpose of tests seems indeed to be the only thing left to the scientist.

And so it is – provided facts *exist, and are available independently of whether or not one considers alternatives to the theory to be tested.* This assumption, on which the validity of the foregoing argument depends in a most decisive manner, I shall call the assumption of the relative autonomy of facts, or the *autonomy principle.* It is not asserted by this principle that the discovery and description of facts is independent of *all* theorizing. But it is asserted that the facts which belong to the empirical content of some theory are available whether or not one considers alternatives to *this* theory. I am not aware that this very important assumption has ever

been explicitly formulated as a separate postulate of the empirical method. However, it is clearly implied in almost all investigations which deal with questions of confirmation and test. All these investigations use a model in which a *single* theory is compared with a class of facts (or observation statements) which are assumed to be 'given' somehow. I submit that this is much too simple a picture of the actual situation. Facts and theories are much more intimately connected than is admitted by the autonomy principle. Not only is the description of every single fact dependent on *some* theory (which may, of course, be very different from the theory to be tested), but there also exist facts which cannot be unearthed except with the help of alternatives to the theory to be tested, and which become unavailable as soon as such alternatives are excluded. This suggests that the methodological unit to which we must refer when discussing questions of test and empirical content is constituted by a *whole set of partly overlapping, factually adequate, but mutually inconsistent theories.* In the present chapter only the barest outlines will be given of such a test model. However, before doing this, I want to discuss an example which shows very clearly the function of alternatives in the discovery of critical facts.

It is now known that the Brownian particle is a perpetual motion machine of the second kind and that its existence refutes the phenomenological second law. Brownian motion therefore belongs to the domain of relevant facts for the law. Now could this relation between Brownian motion and the law have been discovered in a *direct* manner, i.e. could it have been discovered by an examination of the observational consequences of the phenomenological theory that did not make use of an alternative theory of heat? This question is readily divided into two: (1) Could the *relevance* of the Brownian particle have been discovered in this manner? (2) Could it have been demonstrated that it actually *refutes* the second law?

The answer to the first question is that we do not know. It is impossible to say what would have happened if the kinetic theory had not been introduced into the debate. It is my guess, however, that in that case the Brownian particle would have been regarded as an oddity – in much the same way as some of the late Professor Ehrenhaft's astounding effects were regarded as an oddity, and that it would not have been given the decisive position it assumed in contemporary theory. The answer to the second question is simply – No. Consider what the discovery of an inconsistency between the phenomenon of Brownian motion and the second law would

have required. It would have required: (a) measurement of the exact *motion* of the particle in order to ascertain the change in its kinetic energy plus the energy spent on overcoming the resistance of the fluid; and (b) precise measurements of temperature and heat transfer in the surrounding medium in order to establish that any loss occurring there was indeed compensated by the increase in the energy of the moving particle and the work done against the fluid. Such measurements are beyond experimental possibilities;[4] neither the heat transfer nor the path of the particle can be measured with the desired precision. Hence a 'direct' refutation of the second law that considers only the phenomenological theory and the 'facts' of the Brownian motion is impossible. It is impossible because of the structure of the world in which we live and because of the laws that are valid in this world. And as is well known, the actual refutation was brought about in a very different manner. It was brought about via the kinetic theory and Einstein's utilization of it in his calculation of the statistical properties of Brownian motion. In the course of this procedure, the phenomenological theory (T′) was incorporated into the wider context of statistical physics (T) in such a manner that *the consistency condition was violated*, and it was only *then* that crucial experiments were staged (investigations of Svedberg and Perrin).[5]

4. For details see R. Fürth, *Zs. Physik*, Vol. 81, 1933, pp. 143ff.

5. For these investigations (whose philosophical background derives from Boltzmann) see A. Einstein, *Investigation on the Theory of the Brownian Motion*, ed. R. Fürth, New York, 1956, which contains all the relevant papers by Einstein and an exhaustive bibliography by R. Fürth. For the experimental work of J. Perrin, see *Die Atome*, Leipzig, 1920. For the relation between the phenomenological theory and the kinetic theory of von Smoluchowski, see 'Experimentell nachweisbare, der üblichen Thermodynamik widersprechende Molekularphänomene', *Physikalische Zs.*, Vol. 8, 1912, p. 1069, as well as the brief note by K.R. Popper, 'Irreversibility, or, Entropy since 1905', *British Journal for the Philosophy of Science*, Vol. 8, 1957, p. 151, which summarizes the essential arguments. Despite Einstein's epoch-making discoveries and von Smoluchowski's splendid presentation of their consequences (*Oeuvres de Marie Smoluchowski*, Cracow, 1927, Vol. 2, pp. 226ff, 316ff, 462ff and 530ff), the present situation in thermodynamics is extremely unclear, especially in view of the continued presence of some very doubtful ideas of reduction. To be more specific, the attempt is frequently made to determine the entropy balance of a complex *statistical* process by reference to the (refuted) *phenomenological* law after which fluctuations are inserted in an *ad hoc* fashion. For this see my note 'On the Possibility of a Perpetuum Mobile of the Second Kind', *Mind, Matter and Method*, Minneapolis, 1966, p. 409, and my paper 'In Defence of Classical Physics', *Studies in the History and Philosophy of Science* 1, No. 2, 1970.

It seems to me that this example is typical of the relation between fairly general theories, or points of view, and the 'facts'. Both the relevance and the refuting character of decisive facts can be established only with the help of other theories which, though factually adequate,[6] are not in agreement with the view to be tested. This being the case, the invention and articulation of alternatives may have to precede the production of refuting facts. Empiricism, at least in some of its more sophisticated versions, demands that the empirical content of whatever knowledge we possess be increased as much as possible. *Hence the invention of alternatives to the view at the centre of discussion constitutes an essential part of the empirical method.* Conversely the fact that the consistency condition eliminates alternatives now shows it to be in disagreement not only with scientific practice but with empiricism as well. By excluding valuable tests it decreases the empirical content of the theories that are permitted to remain (and these, as I have indicated above, will usually be the theories which were there first); and it especially decreases the number of those facts that could show their limitations. This is how empiricists (such as Newton, or some proponents of what has been called the orthodox interpretation of quantum mechanics) who defend the consistency condition, being unaware of the complex nature of scientific knowledge (and, for that matter, of any form of knowledge) are voiding their favourite theories of empirical content and thus turning them into what they most despise, viz. metaphysical doctrines.[7]

It ought to be mentioned, incidentally, that in 1903, when Einstein started his work in thermodynamics, there existed empirical evidence suggesting that Brownian motion could not be a molecular phenomenon. See F.M. Exner, 'Notiz zu Browns Molekularbewegung', *Ann. Phys.*, No. 2, 1900, p. 843. Exner claimed that the motion was of orders of magnitude beneath the value to be expected on the equipartition principle. Einstein (*Investigations in the Theory of the Brownian Movement*, pp. 63ff, esp. p. 67) gave the following theoretical explanation of the discrepancy: 'since an observer operating with definite means of observation in a definite manner can never perceive the actual path transversed in an arbitrarily small time, a certain mean velocity will always appear to him as an instantaneous velocity. But it is clear that the velocity ascertained thus corresponds to no objective property of the motion under investigation.' See also Mary Jo Nye, *Molecular Reality*, London, 1972, pp. 98ff.

6. The condition of factual adequacy will be removed in Chapter 5.

7. The most dramatic confirmation of the orthodox view which made its empirical nature obvious came by way of Bell's theorem. But Bell was on the side of Einstein, not of Bohr, whom he regarded as an 'obscurantist'. See Jeremy Bernstein, *Quantum Profiles*, Princeton, 1991, pp. 3ff (for Bell's background) and p. 84 (for 'obscurantist').

John Stuart Mill has given a fascinating account of the gradual transformation of revolutionary ideas into obstacles to thought. When a new view is proposed it faces a hostile audience and excellent reasons are needed to gain for it an even moderately fair hearing. The reasons are produced, but they are often disregarded or laughed out of court, and unhappiness is the fate of the bold inventors. But new generations, being interested in new things, become curious; they consider the reasons, pursue them further and groups of researchers initiate detailed studies. The studies may lead to surprising successes (they also raise lots of difficulties). Now nothing succeeds like success, even if it is success surrounded by difficulties. The theory becomes acceptable as a topic for discussion; it is presented at meetings and large conferences. The diehards of the status quo feel an obligation to study one paper or another, to make a few grumbling comments, and perhaps to join in its exploration. There comes then a moment when the theory is no longer an esoteric discussion topic for advanced seminars and conferences, but enters the public domain. There are introductory texts, popularizations; examination questions start dealing with problems to be solved in its terms. Scientists from distant fields and philosophers, trying to show off, drop a hint here and there, and this often quite uninformed desire to be on the right side is taken as a further sign of the importance of the theory.

Unfortunately, this increase in importance is not accompanied by better understanding; the very opposite is the case. Problematic aspects which were originally introduced with the help of carefully constructed arguments now become basic principles; doubtful points turn into slogans; debates with opponents become standardized and also quite unrealistic, for the opponents, having to express themselves in terms which presuppose what they contest, seem to raise quibbles, or to misuse words. Alternatives are still employed but they no longer contain realistic counter-proposals; they only serve as a background for the splendour of the new theory. Thus we do have success – but it is the success of a manoeuvre carried out in a void, overcoming difficulties that were set up in advance for easy solution. An empirical theory such as quantum mechanics or a pseudo-empirical practice such as modern scientific medicine with its materialistic background can of course point to numerous achievements but *any* view and *any* practice that has been around for some time has achievements. The question is whose achievements are better or more important, and *this* question cannot

be answered for there are no realistic alternatives to provide a point of comparison. A wonderful invention has turned into a fossil.

There exist numerous historical examples of the process I have just described, and various authors have commented on it. The most important recent author is Professor Thomas Kuhn. In his *Structure of Scientific Revolutions*,[8] he distinguishes between science and pre-science and, within science, between revolutions and normal science. Pre-science, according to him, is pluralistic throughout and therefore in danger of concentrating on opinions rather than on things (Bacon made a similar point). The two components of mature science perfectly agree with the two stages mentioned above except that Kuhn doubts that science or, for that matter, any activity that claims to produce factual knowledge can do without a normal component. Fossils, he seems to say, are needed to give substance to the debates that occur in the revolutionary component – but he adds that the latter cannot advance without alternatives. Two earlier authors are Mill and Niels Bohr. Mill gives a clear and compelling description of the transition from the early stage of a new view to its orthodoxy. Debates and reasoning, he writes, are features

> belonging to periods of transition, when old notions and feelings have been unsettled and no new doctrines have yet succeeded to their ascendancy. At such times people of any mental activity, having given up their old beliefs, and not feeling quite sure that those they still retain can stand unmodified listen eagerly to new opinions. But this state of things is necessarily transitory: some particular body of doctrine in time rallies the majority round it, organizes social institutions and modes of action conformably to itself, education impresses this new creed upon the new generation *without the mental processes that have led to it* and by degrees it acquires the very same power of compression, so long exercised by the creeds of which it had taken the place.[9]

An account of the alternatives replaced, of the process of replacement, of the arguments used in its course, of the strength of the old views and the weaknesses of the new, not a 'systematic account' but a *historical account*

8. Chicago, 1962.

9. 'Autobiography', quoted from *Essential Works of John Stuart Mill*, ed. M. Lerner, New York, 1965, p. 119; my emphasis.

of each stage of knowledge, can alleviate these drawbacks and increase the rationality of one's theoretical commitments. Bohr's presentation of new discoveries has precisely this pattern; it contains preliminary summaries surveying the past, moves on to the 'present state of knowledge' and ends up by making general suggestions for the future.[10]

Mill's views and Bohr's procedure are not only an expression of their liberal attitude; they also reflect their conviction that a pluralism of ideas and forms of life is an essential part of any rational inquiry concerning the nature of things. Or, to speak more generally: *Unanimity of opinion may be fitting for a rigid church, for the frightened or greedy victims of some (ancient, or modern) myth, or for the weak and willing followers of some tyrant. Variety of opinion is necessary for objective knowledge. And a method that encourages variety is also the only method that is compatible with a humanitarian outlook.* (To the extent to which the consistency condition delimits variety, it contains a theological element which lies, of course, in the worship of 'facts' so characteristic of nearly all empiricism.[11])

10. For a more detailed account see my *Philosophical Papers*, Vol. 1, Chapter 16, section 6.

11. It is interesting to see that the platitudes that directed the Protestants to the Bible are often almost identical with the platitudes which direct empiricists and other fundamentalists to *their* foundation, viz. experience. Thus in his *Novum Organum* Bacon demands that all preconceived notions (aphorism 36), opinions (aphorisms 42ff), even *words* (aphorisms 59, 121), 'be adjured and renounced with firm and solemn resolution, and the understanding must be completely freed and cleared of them, so that the access to the kingdom of man, which is founded on the sciences, may resemble that to the kingdom of heaven, where no admission is conceded, except to children' (aphorism 68). In both cases 'disputation' (which is the consideration of alternatives) is criticized, in both cases we are invited to dispense with it, and in both cases we are promised an 'immediate perception', here, of God, and there of Nature. For the theoretical background of this similarity see my essay 'Classical Empiricism', in R.E. Butts (ed.), *The Methodological Heritage of Newton*, Oxford and Toronto, 1970. For the strong connections between Puritanism and modern science see R.T. Jones, *Ancients and Moderns*, California, 1965, Chapters 5–7. A thorough examination of the factors that influenced the rise of modern empiricism in England is found in R.K. Merton, *Science, Technology and Society in Seventeenth Century England*, New York, 1970 (book version of the 1938 article).

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There is no idea, however ancient and absurd, that is not capable of improving our knowledge. The whole history of thought is absorbed into science and is used for improving every single theory. Nor is political interference rejected. It may be needed to overcome the chauvinism of science that resists alternatives to the status quo.

This finishes the discussion of part one of counterinduction dealing with the invention and elaboration of hypotheses inconsistent with a point of view that is highly confirmed and generally accepted. The result was that a thorough examination of such a point of view may involve incompatible alternatives so that the (Newtonian) advice to postpone alternatives until after the first difficulty has arisen means putting the cart before the horse. A scientist who is interested in maximal empirical content, and who wants to understand as many aspects of his theory as possible, will adopt a pluralistic methodology, he will compare theories with other theories rather than with 'experience', 'data', or 'facts', and he will try to improve rather than discard the views that appear to lose in the competition.[1] For the alternatives, which he needs to keep the contest going, may be taken from the past as well. As a matter of fact, they may be taken from wherever one is able to find them – from ancient myths and modern prejudices; from the lucubrations of experts and from the fantasies of cranks. The whole history of a subject is utilized in the attempt to improve its most recent and most 'advanced' stage. The separation between the history of a science, its philosophy and the science itself dissolves into thin air and so does the separation between science and non-science.[2]

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1. It is, therefore, important that the alternatives be set against each other and not be isolated or emasculated by some form of 'demythologization'. Unlike Tillich, Bultmann and their followers, we should regard the world-views of the Bible, the Gilgamesh epic, the Iliad, the Edda, as fully fledged *alternative cosmologies* which can be used to modify, and even to replace, the 'scientific' cosmologies of a given period.

2. An account and a truly humanitarian defence of this position can be found in J.S. Mill's *On Liberty*. Popper's philosophy, which some people would like to lay on us as the one and only humanitarian rationalism in existence today, is but a pale reflection of Mill. It is specialized, formalistic and elitist, and devoid of the concern for individual happiness that is such a characteristic feature of Mill. We can understand its peculiarities when we consider (a) the background of logical positivism, which plays an important role in the

This position, which is a natural consequence of the arguments presented above, is frequently attacked – not by counter-arguments, which would be easy to answer, but by rhetorical questions. 'If any metaphysics goes,' writes Dr Hesse in her review of an earlier essay of mine,[3] 'then the question arises why we do not *go back* and exploit the objective criticism of modern science available in Aristotelianism, or indeed in Voodoo?' – and she insinuates that a criticism of this kind

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Logic of Scientific Discovery, (b) the unrelenting puritanism of its author (and of most of his followers), and when we remember the influence of Harriet Taylor on Mill's life and on his philosophy. There is no Harriet Taylor in Popper's life. The foregoing arguments should also have made it clear that I regard proliferation not just as an 'external catalyst' of progress, as Lakatos suggests in his essays ('History of Science and Its Rational Reconstructions', *Boston Studies*, Vol. 8, p. 98; 'Popper on Demarcation and Induction', MS, 1970, p. 21), but as an essential part of it. Ever since 'Explanation, Reduction, and Empiricism' (*Minnesota Studies*, Vol. 3, Minneapolis, 1962), and especially in 'How to Be a Good Empiricist' (*Delaware Studies*, Vol. 2, 1963), I have argued that alternatives increase the empirical content of the views that happen to stand in the centre of attention and are, therefore, '*necessary* parts' of the falsifying process (Lakatos, 'History', fn. 27, describing his own position). In 'Reply to Criticism' (*Boston Studies*, Vol. 2, 1965) I pointed out that 'the principle of proliferation not only recommends invention of *new* alternatives, it also prevents the elimination of *older* theories which have been refuted. The reason is that such theories contribute to the content of their victorious rivals' (p. 224). This agrees with Lakatos' observation of 1971 that 'alternatives are not merely catalysts, which can later be removed in the rational reconstruction' ('History', fn. 27), *except* that Lakatos attributes the psychologistic view to me and my *actual* views to himself. Considering the argument in the text, it is clear that the increasing separation of the history, the philosophy of science and of science itself is a disadvantage and should be terminated in the interest of all these three disciplines. Otherwise we shall get tons of minute, precise, but utterly barren results.

3. Mary Hesse, *Ratio*, No. 9, 1967, p. 93; cf. B.F. Skinner, *Beyond Freedom and Dignity*, New York, 1971, p. 5: 'No modern physicist would turn to Aristotle for help.' This is neither true, nor would it be an advantage if it were true. Aristotelian ideas influenced research long after they had allegedly been removed by early modern astronomy and physics – any history of 17th- or 18th-century science will show that (example: John Heilbronn's marvellous *Electricity in the 17th and 18th Centuries*, Berkeley and Los Angeles, 1979). They resurfaced in biology, in the thermodynamics of open systems and even in mathematics. Aristotle's theory of locomotion (which has the consequence that a moving object has no precise length and that an object having a precise location must be at rest) was more advanced than the Galilean view and showed that ideas which in our time emerged from empirical research can be obtained by a careful analysis of the problems of the continuum (details on this point in Chapter 8 of my *Farewell to Reason*, London, 1987). Here as elsewhere the propagandists of a naive scientism give themselves the air of presenting arguments when all they do is spread unexamined and ill-conceived rumours.

would be altogether laughable. Her insinuation, unfortunately, assumes a great deal of ignorance in her readers. Progress was often achieved by a 'criticism from the past', of precisely the kind that is now dismissed by her. After Aristotle and Ptolemy, the idea that the earth moves – that strange, ancient, and 'entirely ridiculous',[4] Pythagorean view – was thrown on the rubbish heap of history, only to be revived by Copernicus and to be forged by him into a weapon for the defeat of its defeaters. The Hermetic writings played an important part in this revival, which is still not sufficiently understood,[5] and they were studied with care by the great Newton himself.[6] Such developments are not surprising. No idea is ever examined in all its ramifications and no view is ever given all the chances it deserves. Theories are abandoned and superseded by more fashionable accounts long before they have had an opportunity to show their virtues. Besides, ancient doctrines and 'primitive' myths appear strange and nonsensical only because the information they contain is either not known, or is distorted by philologists or anthropologists unfamiliar with the simplest physical, medical or astronomical knowledge.[7] Voodoo, Dr

4. Ptolemy, *Syntaxis*, quoted after the translation of Manitius, *Des Claudius Ptolemaeus Handbuch der Astronomie*, Vol. 1, Leipzig, 1963, p. 18.

5. For a positive evaluation of the role of the hermetic writings during the Renaissance cf. F. Yates, *Giordano Bruno and the Hermetic Tradition*, London, 1963, and the literature given there. For a criticism of her position see the articles by Mary Hesse and Edward Rosen in Vol. 5 of the *Minnesota Studies for the Philosophy of Science*, ed. Roger Stuewer, Minneapolis, 1970; R.S. Westman and J.E. McGuire, *Hermeticism and the Scientific Revolution*, Los Angeles, Clark Memorial Library, 1977, as well as Brian Vickers, *Journal of Modern History*, 51, 1979.

6. Cf. J.M. Keynes, 'Newton the Man', in Essays and Sketches in Biography, New York, 1956, and, in much greater detail, McGuire and Rattansi, 'Newton and the "Pipes of Pan"', *Notes and Records of the Royal Society*, Vol. 21, No. 2, 1966, pp. 108ff. For more detailed accounts cf. Frank Manuel, *The Religion of Isaac Newton*, Oxford, 1974, R.S. Westfall's monumental biography, *Never at Rest*, Cambridge, 1980, with literature, as well as Chapters X and XI of R. Popkin, *The Third Force in Seventeenth-Century Thought*, Leiden and New York, 1992.

7. For the scientific content of some myths see C. de Santillana, *The Origin of Scientific Thought*, New York, 1961, especially the Prologue. 'We can see then', writes de Santillana, 'how so many myths, fantastic and arbitrary in semblance, of which the Greek tale of the Argonauts is a late offspring, may provide a terminology of image motifs, a kind of code which is beginning to be broken. It was meant to allow those who knew (a) to determine unequivocally the position of given planets in respect to the earth, to the firmament, and to one another; (b) to present what knowledge there was of the fabric of the world in the

Hesse's *pièce de resistance*, is a case in point. Nobody knows it, everybody uses it as a paradigm of backwardness and confusion. And yet Voodoo has a firm though still not sufficiently understood material basis, and a study of its manifestations can be used to enrich, and perhaps even to revise, our knowledge of physiology.[8]

An even more interesting example is the revival of traditional medicine in Communist China. We start with a familiar development:[9] a great country with great traditions is subjected to Western domination and is exploited in the customary way. A new generation recognizes or thinks it recognizes the material and intellectual superiority of the West and traces it back to science. Science is imported, taught, and pushes aside all traditional elements. Scientific chauvinism triumphs: 'What is compatible with science should live, what is not compatible with science, should

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form of tales about "how the world began". There are two reasons why this code was not discovered earlier. One is the firm conviction of historians of science that science did not start before Greece and that scientific results can only be obtained with the scientific method as it is practised today (and as it was foreshadowed by Greek scientists). The other reason is the astronomical, geological, etc., ignorance of most Assyriologists, Aegyptologists, Old Testament scholars, and so on: the apparent primitivism of many myths is just the reflection of the primitive astronomical, biological, etc., etc., knowledge of their collectors and translators. Since the discoveries of Hawkins, Marshack, Seidenberg, van der Waerden (*Geometry and Algebra in Ancient Civilizations*, New York, 1983) and others we have to admit the existence of an international palaeolithic astronomy that gave rise to schools, observatories, scientific traditions and most interesting theories. These theories, which were expressed in sociological, not in mathematical, terms, have left their traces in sagas, myths, legends, and may be reconstructed in a twofold way, by going *forward* into the present from the material remains of Stone Age astronomy such as marked stones, stone observatories, etc., and by going *back* into the past from the literary remains which we find in sagas, legends, myths. An example of the first method is A. Marshack, *The Roots of Civilization*, New York, 1972. An example of the second is de Santillana-von Dechend, *Hamlet's Mill*, Boston, 1969.

8. See Chapter 9 of Lévi-Strauss, *Structural Anthropology*, New York, 1967. For the physiological basis of Voodoo see C.R. Richter, 'The Phenomenon of Unexplained Sudden Death', in Gantt (ed.), *The Physiological Basis of Psychiatry*, as well as W.H. Cannon, *Bodily Changes in Pain, Hunger, Fear and Rage*, New York, 1915, and ' "Voodoo" Death', in *American Anthropologist*, n.s., XLIV, 1942. The detailed biological and meteorological observations made by so-called 'primitives' are reported in Lévi-Strauss, *The Savage Mind*, London, 1966.

9. R.C. Croizier, *Traditional Medicine in Modern China*, Cambridge, Mass., 1968. The author gives a very interesting and fair account of developments with numerous quotations from newspapers, books, pamphlets, but is often inhibited by his respect for 20th-century science.

die'.[10] 'Science' in this context means not just a specific method, but all the results the method has so far produced. Things incompatible with the results must be eliminated. Old-style doctors, for example, must either be removed from medical practice, or they must be re-educated. Herbal medicine, acupuncture, moxibustion and the underlying philosophy are a thing of the past, no longer to be taken seriously. This was the attitude up to about 1954, when the condemnation of bourgeois elements in the Ministry of Health started a campaign for the revival of traditional medicine. No doubt the campaign was politically inspired. It contained at least two elements, viz. (1) the identification of Western science with bourgeois science and (2) the refusal of the party to exempt science from political supervision[11] and to grant experts special privileges. But it provided the counterforce that was needed to overcome the scientific chauvinism of the time and to make a plurality (actually a duality) of views possible. (This is an important point. It often happens that parts of science become hardened and intolerant so that proliferation must be enforced from the outside, and by political means. Of course, success cannot be guaranteed – see the Lysenko affair. But this does not remove the need for non-scientific controls on science.)

Now this politically enforced dualism has led to most interesting and puzzling discoveries both in China and in the West and to the realization that there are effects and means of diagnosis which modern medicine cannot repeat and for which it has no explanation. It revealed sizeable lacunae in Western medicine. Nor can one expect that the customary scientific approach will eventually find an answer. In the case of herbal medicine the approach consists of two steps.[12] First, the herbal concoction is analysed into its chemical constituents. Then the *specific* effects of each constituent are determined and the total effect on a particular organ explained on their basis. This neglects the possibility that the herb, taken in its entirety, changes the state of the *whole* organism and that it is this new state of the whole organism rather than a specific part of the herbal concoction, a 'magic bullet', as it were, that cures the diseased organ.

10. Chou Shao, 1933, as quoted in Croizier, op. cit., p. 109. See also D.W.Y. Kwok, *Scientism in Chinese Thought*, New Haven, 1965.

11. For the tensions between 'red' and 'expert' see F. Schurmann, *Ideology and Organization in Communist China*, Berkeley, 1966.

12. See M.B. Krieg, *Green Medicine*, New York, 1964.

Here as elsewhere knowledge is obtained from a multiplicity of views rather than from the determined application of a preferred ideology. And we realize that proliferation may have to be enforced by non-scientific agencies whose power is sufficient to overcome the most powerful scientific institutions. Examples are the Church, the State, a political party, public discontent, or money: the best single entity to get a modern scientist away from what his 'scientific conscience' tells him to pursue is still the *dollar* (or, more recently, the Swiss franc).

Pluralism of theories and metaphysical views is not only important for methodology, it is also an essential part of a humanitarian outlook. Progressive educators have always tried to develop the individuality of their pupils and to bring to fruition the particular, and sometimes quite unique, talents and beliefs of a child. Such an education, however, has very often seemed to be a futile exercise in day-dreaming. For is it not necessary to prepare the young for life *as it actually is*? Does this not mean that they must learn *one particular set of views* to the exclusion of everything else? And, if a trace of their imagination is still to remain, will it not find its proper application in the arts or in a thin domain of dreams that has but little to do with the world we live in? Will this procedure not finally lead to a split between a hated reality and welcome fantasies, science and the arts, careful description and unrestrained self-expression? The argument for proliferation shows that this need not happen. It is possible to *retain* what one might call the freedom of artistic creation *and to use it to the full*, not just as a road of escape but as a necessary means for discovering and perhaps even changing the features of the world we live in. This coincidence of the part (individual man) with the whole (the world we live in), of the purely subjective and arbitrary with the objective and lawful, is one of the most important arguments in favour of a pluralistic methodology. For details the reader is advised to consult Mill's magnificent essay *On Liberty*.[13]

13. See my account of this essay in Vol. 1, Chapter 8 and Vol. 2, Chapter 4 of my *Philosophical Papers*. See also Appendix 1 of the present essay.

No theory ever agrees with all the facts in its domain, yet it is not always the theory that is to blame. Facts are constituted by older ideologies, and a clash between facts and theories may be proof of progress. It is also a first step in our attempt to find the principles implicit in familiar observational notions.

Considering now the invention, elaboration and the use of theories which are inconsistent, not just with other theories, but even with *experiments, facts, observations*, we may start by pointing out that *no single theory ever agrees with all the known facts in its domain.* And the trouble is not created by rumours, or by the result of sloppy procedure. It is created by experiments and measurements of the highest precision and reliability.

It will be convenient, at this place, to distinguish two different kinds of disagreement between theory and fact: numerical disagreement, and qualitative failures.

The first case is quite familiar: a theory makes a certain numerical prediction and the value that is actually obtained differs from the prediction made by more than the margin of error. Precision instruments are usually involved here. Numerical disagreements abound in science. They give rise to an 'ocean of anomalies' that surrounds every single theory.[1]

Thus the Copernican view at the time of Galileo was inconsistent with facts so plain and obvious that Galileo had to call it 'surely false'.[2] 'There is no limit to my astonishment,' he writes in a later work,[3] 'when I reflect that Aristarchus and Copernicus were able to make reason so conquer sense that, in defiance

1. For the 'ocean' and various ways of dealing with it, see my 'Reply to Criticism', *Boston Studies*, Vol. 2, 1965, pp. 224ff.

2. Galileo Galilei, *The Assayer*, quoted in S. Drake and C.D. O'Malley (eds), *The Controversy on the Comets of 1618*, London, 1960, p. 185. The 'surely false' refers to the condemnation by Church authorities. But, as will be explained in the course of the book and especially in Chapter 13, the condemnation was based in part on the 'philosophical absurdity' of the idea of a moving earth, i.e. on its empirical failures and its theoretical inadequacy. See also the next quotation and footnote. 'As to the system of Ptolemy', writes Galileo on this point (p. 184), 'neither Tycho, nor other astronomers, nor even Copernicus could clearly refute it, inasmuch as a most important argument taken from the movement of Mars and Venus always stood in their way.' The 'most important argument' and Galileo's resolution are discussed in Chapters 9 and 10.

3. Galileo Galilei, *Dialogue Concerning the Two Chief World Systems*, Berkeley, 1953, p. 328.